satisfactory velocity. Its subsequent sailing flight until it again increases its rate of speed by flapping, I would suggest to be merely a utilisation of this original *vis viva* to the utmost possible advantage, the ascending and descending movements of the bird being nothing more than a change from actual to potential energy, and *vice versâ*. Suppose, for the sake of simplicity, that the wind is dead ahead, and that the bird commences sailing horizontally with a certain *vis viva*. With this, by fixing its wings so as to present inclined planes to the direction of the wind, it is able to rise to a certain height, the velocity decreasing in some ratio to the ascent, and if the highest point capable of being reached is attained, the bird for the instant comes to rest; up to this moment the actual energy has been gradually changing into potential, and the bird gaining thereby a position of advantage. It is, however, extremely rare that this position is attained—most frequently the horizontal velocity is only partially destroyed. The planes of the wings being now changed with reference to the direction of the wind, the bird begins to descend; the potential energy is transformed into actual, and velocity is acquired, to be again changed into potential, and so on until it becomes necessary to renew it. The line of flight, therefore, of an albatross going directly against the wind consists of a series of undulations, the summits of which correspond to the instants of least relative velocities, or positions of greatest potential advantage; whilst the lowest points correspond to the instants of greatest relative velocity and least potential advantage.

During all this time *vis viva* is of course being extracted by the resistance of the wind, and the velocity after a while is so diminished that the bird loses its power of rising to a satisfactory position of advantage. It is then that flapping recommences and new power of flight is acquired. When it is remembered that the weight of a Cape albatross varies from 16 lb. to 20 lb., and the stretch of wings from 10 to 12 feet, it will be evident how great is the potential energy of such a bird at the height say of 100 feet, and also how complete is its power of utilising that energy. The question may be asked, how long will it be before 2,000 foot-pounds of work have been extracted by air moving at the rate of sixty miles an hour? for until it has been extracted, or nearly so, the sailing flight of the albatross need not cease. By means of a suitable mechanism for changing the inclination of the wing planes every few seconds, the sailing of the albatross, I believe, might be simulated without great difficulty. It is generally supposed that the stronger the wind the greater is the power of sailing-flight. In the special instance referred to, viz., that of sailing directly in the teeth of the wind, this is not the case. A good breeze is evidently better than either a very strong wind or a calm. In the one case, a too great resistance destroys the *vis viva* too rapidly; in the other, the bird suffers from a want of sufficient resistance, very much as a kite does during a calm.

In sailing in any other direction a violent wind may more or less aid the flight, and the velocity attained in some instances be enormous and very deceptive. It is this element, viz., the velocity acquired by sailing obliquely with the wind, that is so difficult for the eye to eliminate in estimating the actual power of the bird to sail against the wind. In flying with the wind, the resistance to the stroke being greater, the necessary speed may be more rapidly acquired and with fewer strokes, provided the bird has the requisite strength. But, as might naturally be supposed, sailing directly with the wind for any considerable distance is rarely or never seen, the bird not finding sufficient resistance in the air for its support.

From what has been said it will appear that the superior sailing power of the albatross, in comparison with other birds, is due—

(1) To its ability to acquire readily very great *vis viva* by means of its extremely powerful wings.

(2) To its almost perfect power of utilising this *vis viva* for the purpose of ascending or descending, *i.e.* of changing from a position of greatest actual to greatest potential energy and *vice versâ*, with least loss of power through resistance of the air.

The above implies an extraordinary rigidity as well as absence of concavity of the wings, by which the bird is enabled to hold them in their place like two rigid planes, and thereby present their surface to the wind under the most favourable circumstances possible. The tremulous movement seen at the tips appears to be nothing more than vibrations due to the want of absolute rigidity in the pinions. The above suggestions, if tenable, furnish an explanation also of the flight of the flying fish—the undulatory motion, or rising over the crest of a wave, which has puzzled so many casual observers, being merely a change of some of the *vis viva* of its flight into potential energy. This means necessarily a loss of velocity depending on the amount of rise, and implies the power of the fish to change its wing planes so as to ascend or descend. The original *vis viva* has of course been created by a preliminary rush through the water before emerging.

It will be seen from what has been said that the principle suggested, rightly or wrongly, as fully explaining the flight of the albatross, is that of a body—gifted with the most perfect power of placing itself in a position of advantage—sliding up and down inclined planes under the most perfect conditions possible.

R. A.

The Use of the Words "Weight" and "Mass"

In the review of Dr. Guthrie's "Electricity and Magnetism" (Nature, vol. xiii. p. 263) the following words occur in reference to Dr. Guthrie's definition of the absolute unit of electric resistance: "Here, irrespective of other considerations, there is the fundamental error of using the term *weight* instead of *mass*."

It is very unfortunate that the word "weight" is ambiguous; and that the ambiguity is actually so great as to lead to all but universal confusion of ideas. It is *not* really improper to use *weight* as synonymous with *mass*, and, had Dr. Guthrie meant to refer to *mass*, his using the term weight would not have constituted any fundamental error. He would only have been using an old ambiguous word in the more authoritatively established of its two common meanings. By an Act of Parliament (18th and 19th of Victoria, Chapter 72, July 30, 1855) for the special purpose of establishing standard weights and measures, it is enacted that a certain piece of platinum referred to as a "weight of platinum" shall be denominated the Imperial Standard Pound Avoirdupois, and shall be deemed to be the only standard of weight from which all other weights and other measures having reference to weight shall be derived, computed, and ascertained. The gravity of a mass, or of a piece of matter, is not once named, or in any way referred to, in the Act as a thing for which a standard is meant to be established by that Act, nor is the word *force* or the notion of force put forward in any way in the Act. Thus the meaning attributed in the Act to the word *weight* is the same as is distinctly expressed in scientific language by *mass*.

However, on turning to Dr. Guthrie's book itself, I found a striking example of the troublesome perplexity which is involved in the ambiguity of the language in common use. A few lines below the passage touched on by the reviewer, the following sentence and appended note occur. (Text), "From the work done by the current in the experimental wire, the resistance in that wire is found, and this resistance is considered unity when the above measures are units, namely, 1 second time, 1 meter space, and 1 gram weight or force." (Note appended), "The force actually taken as unity is $\frac{1}{9 \cdot 81}$ gram, for this force acting on 1 gram for 1 second will give it a velocity of 1 meter a second." The text and the note are utterly irreconcilable. The confusion is complete.

I do not say that no one can possibly understand the subject with the common nomenclature; but I do say, from considerable experience in Glasgow University, where we are in the habit of using the absolute or kinetic system of force-measurement in *all* our calculations with the students of the Natural Philosophy Class, that it is extremely difficult to explain, with the old nomenclature, the beautiful, and in itself simple, kinetic system of Gauss, together with its connection with the gravitation system of force-measurement.

This session, however, I have found a very great simplification in adopting a suggestion of Prof. James Thomson to do away with the word *weight* altogether in cases in which its employment would involve ambiguity. He would still readily use the name, a pound weight, for the standard piece of iron or brass used in weighing; and would continue, so long as our present non-decimal system is maintained, to use the commercial term, a hundredweight of iron, meaning a certain quantity or mass of iron. But he has proposed that when we mean *mass* we should avoid the word weight as far as possible and use the word mass, and that where we mean downward force due to gravitation, called by Dr. Guthrie and his reviewer, *weight*, we should use the word *gravity*. Thus we may speak of a one pound force, or we may say "the gravity of a pound," but never "the weight of a pound." We can scarcely get rid altogether of connecting the idea of heaviness with the word weight, nor would our dictionaries at present allow us to do so; but it is quite proper to feel that, in speaking of a certain weight as being too great to

be resisted by a certain chain, we are using a colloquial and inaccurate expression, like calling a door *heavy* when we are not attempting the feat of Samson, but merely opening or shutting it, turning it on its well-oiled hinges.

During the present session we have aided ourselves in Glasgow with four very important helps to the teaching of the kinetic system of force-measurement. One is the improvement in nomenclature just referred to. The second is the use of names for the kinetic units of force. The British Association has sanctioned the use of the name *Dyne* for the kinetic unit of force founded on the centimetre, gramme, and second, as units of length, mass, and time respectively. Prof. James Thomson has given the name *Poundal* for the British kinetic unit of force founded on the foot, pound, and second. The third help is the construction by Prof. Thomson, for the first time, so far as I know, of spring balances for indicating poundals and kilodynes. The fourth aid is Dr. Everett's admirable book on the C. G. S. system of units. J. T. BOTTOMLEY

University of Glasgow, Feb. 7

Seasonal Order of Colour in Flowers

I AM very much obliged to Mr. Buchan for his elaborate paper in NATURE, vol. xiii. p. 249, on the Flowering of Spring Plants (see my query, NATURE, vol. xiii. p. 129). Although agreeing with Mr. Pryor that the blue is anticipated by various other colours, yet I think that the method of inquiry by averages is the only basis we can go upon; and that is the plan I have adopted for some time. I have now a carefully-assorted collection of hyacinths, and I see that the blue and white are coming out nearly together, the red showing as yet no colour whatever. What would be the action of light upon blue or red flowers, if the blue or red ray was carefully excluded, if this could be done? Would the flower thrive, and if so, would its colour be much altered? C. E. HERON ROGERS

Retford, Notts, Feb. 7

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—Herr Julius Schmidt publishes (in *Astron. Nach.*, No. 2,074) the results of observations of variable stars made at Athens in 1875, amongst which the following may be noted :—

1. 34 Bootis, a star to which he had directed attention some years since, as certainly variable though observed with difficulty on account of proximity to ϵ, was found to be at a maximum on April 26—a good determination. In 1872 he assigned a period of 369 days from six observed maxima, commencing 1867, July 31, and as many minima, the first, 1867, November 18. Between the maxima of 1867 and 1875, we should have eight periods of about 353 days. The mean place of this star for the beginning of the present year is in R.A. 14h. 37m. 32s., N.P.D., 62° 54'·1.

2. Mira Ceti. Three curves drawn from comparisons of this star with α and γ Ceti and α Piscuim gave the date of maximum, February 27·5, March 1 and 3 respectively, of which the latter is preferred. Calculating from the formula of sines in Schönfeld's second catalogue, the maximum of 1875 is fixed to February 24·2. Observed minimum, October 30.

3. η Geminorum.—The variability of this star was detected by Schmidt in 1865, and has since been confirmed by Schönfeld, who found for the brighter phase small and not very regular fluctuations, but for the minima a regular diminution and increase, the first continuing about six weeks, and the last perhaps rather longer. This is in near agreement with Schmidt's previous deductions. He had found by comparison with μ Geminorum that η at times remained constant for several months about the maxima, of which, writing in 1869, he states he had been unable to assign the dates. In 1875, however, two maxima were noted, Feb. 25 and Sept. 23; showing an interval of 210 days. The period assigned in the last Manheim catalogue is 229·1 days. This star is of a deep yellow colour. Variation between extremes of 3·2 and 4·2.

4. ϵ Aurigæ.—Schmidt collects the results of his comparisons of the relative brightness of ϵ and η Aurigæ, between the years 1843-1875. The star is irregularly variable within somewhat narrow limits.

5. u Herculis.—The principal period appears to be about 38·7 days, but according to Schmidt (A. N. 2,075) the curve exhibits waves of about twelve hours' duration, which are of the greatest depth at the principal minimum, and comparatively shallow at the maximum, and he has given a figure explanatory of what he considers to have been the law of variation between 1875, July 4, and Aug. 29. So unique a case appears to require further investigation.

6. g Herculis.—This reddish-yellow variable was discovered by Mr. Baxendell in 1857, and has been carefully observed by Schmidt. The period, according to Schönfeld, has varied between 40 and 125 days, the star thus resembling in the great irregularity of period the well-known R Scuti, which was discovered by Pigott in 1795. Last year Schmidt's comparisons showed three maxima and two minima, indicating periods 77, 73, and 77 days. The variation extends through little over one magnitude.

7. α Cassiopeæ.—Of this star Schmidt remarks that the fluctuations of brightness in 1875 were not greater than in the cases of other stars, which are not yet placed upon the variable list.

8. T Coronæ Borealis (Nova 1866).—Mostly ninth magnitude, or rather fainter; exhibiting sensible variation, but to very small amount.

9. R Scuti.—Observed maxima on October 12 and December 8 give the short period of 57 days. The period entered in Schönfeld's second catalogue is 71·1 days. There are great irregularities in the case of this star, not only in the period but in the degree of brightness at both maximum and minimum; the former has been noted between 4·7 and 5·7, and the latter between 6·0 and 8·5.

MINOR PLANETS.—No. 131, *Vala*, discovered by Prof. Peters at Clinton, U.S., 1873, May 24, has so far been unsuccessfully sought at Pola and Berlin between limits of − 30m. and + 15m. in respect to the place of the ephemeris apparently founded on Stockwell's elements; the longitude of perihelion in this orbit differs materially from that given by Knorre's earlier calculation, and possibly a misprint or error of transcription may be the cause of the difficulty.—Prof. Tietjen notifies that the ephemeris of No. 141, *Lumen*, in the *Berliner Jahrbuch* for 1878, is vitiated by an error in *Astr. Nach.*, No. 2,030, where ω is substituted for π; the habit of some computers of giving the orbital angle between perihelion and node, instead of the longitude of perihelion itself, is certainly not without its inconvenience, and this is more particularly the case with early orbits of comets.—No. 156, discovered by Palisa, 1875, Nov. 22, has been named *Xanthippe*.—New elements of No. 158 give a period of 1,889 days, or 5·17 years.

THE SATURNIAN SATELLITE, HYPERION.—Observations of this faint object made with the 26-inch refractor of the U.S. Naval Observatory on forty nights between 1875, June 16 and Nov. 25, appear in No. 2,076 of the *Astron. Nach.* It is stated that the observations were generally made with difficulty. Prof. Asaph Hall acknowledges his obligations to Mr. Marth for his ephemerides of the satellites of Saturn, by which he has endeavoured to facilitate identification of these objects, and which could only have been prepared at an expenditure of much time and trouble.

THE DATE OF EASTER

WE revert to this subject with the view to reproduce the arithmetical rule to find Easter Sunday in the Gregorian Calendar, which was first given by the